

## ***Interactive comment on “Black carbon concentration and deposition estimations in Finland by the regional aerosol-climate model REMO-HAM” by A. I. Hienola et al.***

**A. I. Hienola et al.**

anca.hienola@fmi.fi

Received and published: 19 December 2012

Answer to Anonymous Referee #2 comments concerning our manuscript “Black carbon concentration and deposition estimations in Finland by the regional aerosol-climate model REMO-HAM” by A. I. Hienola et al., ACPD 12, C8471–C8472, 2012

We thank the referee for relevant and useful comments. In this document, we number the statements from the report. Our replies follow immediately bellow each statement.

1. The authors describe the performance of the model REMO-HAM with respect to reproducing black carbon concentrations at near surface levels at five Finish measurement stations. In addition, BC deposition over Finland and in particular on snow is cal-

C10757

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



culated. While the structure of the paper is good and the presentation of results nicely done, the frame of this study, meaning the chosen time period and the geographical extension of considered BC emissions, needs justification and the study lacks more thorough interpretation to give the results and interpretations credibility. General comments (1) It is unclear why the year 2005 is chosen when only two of the five stations have C8940 data available for this year. Based on Table 2, for the year 2008, all stations have recorded BC data. The authors state (p. 24402, l. 22 ff) that this study is only to be taken quantitatively. What do the authors hope to achieve with this? In which way do the results enhance our understanding of BC concentrations and deposition over Finland especially as long-range transport is not discussed?

Year 2005 was chosen, as it was the closest to the AEROCOM emission inventory (2000). The comparison of the model simulation to Hyytiälä measurements is of course completely quantitative, but as the other stations' data is from different years, the comparison should be regarded more qualitative than quantitative. However, the year-to-year variation of the annual (or even monthly) average BC levels is not very big, so that the comparison far from useless. Furthermore, it gives indication of the BC levels in different parts of the country (note the big difference between Lapland and southern Finland) and shows that the model reproduces the north-south gradient rather well. We have now made this clearer in the revised ms.

(2) It is not clear why emissions outside of Finland and associated analysis on long-range transport (e.g., back trajectories) are neglected.

We have introduced the discussion on long range transported black carbon in the subsection 3.2.3 in the revised version of the manuscript (pages 16-18). Back trajectories are presented in Figs.12-14. In order to assess the contribution of the over border transported BC, we conducted an analysis where the BC emissions in Finland were switched off, presented in the same subsection. Figs. 15 and 16 are relevant to this discussion. In short, we demonstrated that the lack of local emission sources (residential wood burning) is the dominant source of model underestimation, although cases

of very high measured BC concentrations not captured by the model appear to be long-range transport dominated.

(3) Regarding the interpretation of the results: Even though the focus is put on BC, reference to the performance of the model with respect to other aerosol chemical species such as e.g., sulfate could be given for comparison and for a more profound interpretation of the results.

Last paragraph in subsection 2.1. discusses the work done by Pietikäinen et al (2012) and the process of REMO-HAM validation, in terms of aerosol number concentration, size distribution and gas phase sulfur dioxide concentrations.

(4) In sect. 3.1 results are only reported but hardly any interpretation is given.

We have added some discussion, pointing out that even though the comparison to other stations than Hyytiälä is not completely quantitative, we can very likely interpret the model-data differences to be mostly caused by the same reasons as in Hyytiälä, and that the model gets the north-south gradient satisfactorily.

(5) More information regarding the specific sources of measured BC concentrations will help to identify the reason for the discrepancies between observations and model results. Does BC come of fossil fuel combustion or biomass burning? Are measurements of tracers like levoglucosan or potassium available to identify the BC origin?

We have added discussion on the presence of levoglucosan in the subsection 3.2.3 in the revised version of the manuscript (pages 16-18). The levoglucosan levels support our interpretation of missing wood burning emissions as being a large source of model-data discrepancies.

(6) Were there any fires (not only in Finland) detected that are responsible for high BC concentrations?

Wildfires are detected every year in Europe and former USSR. Wildfire smoke events are not very frequent in Finland and last only few days at a time, and, although they

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

might influence the concentrations measured at higher end, they should not have a drastic effect on the median.

(7) Were certain seasons especially cold generating the need for more domestic heating? In how far does the emission inventory consider these sources and their variance? Some more detailed elaboration on potentially missing sources in the inventory is necessary beyond what is stated in the last sentence of sect. 3.2.2.

Late autumn to early spring is naturally much colder in Finland than the rest of the year, generating (increased) need for domestic heating. We have added discussion of different emission inventories in subsection 2.1, second last paragraph.

Specific Comments (8) BC and soot are used like synonyms throughout the paper even though they do not refer to identical substances. I recommend sticking to the usage of BC for the scope of this paper.

We use only BC in the last version of the manuscript.

(9) p. 24397, l. 11ff: the two major UNEP and WMO reports from 2011 regarding the climate effects of reducing BC emissions should be mentioned. (And please not that BC is not only of interest for atmospheric scientists, it has gained much political and public interest within the last years and especially this year (2012)).

UNEP and WMO added.

(10) Model description in general: Some more details on the geographical area considered and the vertical resolution are needed.

Figure 1 and the corresponding text in subchapter 2.1.

(11) p. 24400, l. 4: “. . . although many parts have been. . .” If these changes are relevant for this study, state them or refer to a respective calculation.

Here, we are giving a general model description, and the changes are relevant because of the smaller grid compared to ECHAM (that's why they have been made in the first

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



place), but it is of course impossible to say how big an effect they might have for e.g. modeled BC concentrations. We don't think it is useful to list all changes in model physics compared with ECHAM4, details can be found in the references given.

(12) p. 24401 Experimental data: please add information on the location categories of the stations such as urban, semi-urban, rural or remote

Subchapter 2.2 includes now a better characterization of the stations.

(13) p. 24402, l. 13: The time resolution of 1 minute seems extremely high for BC measurements especially for places like Pallas where no high concentrations are expected.

Even if the instrument measures and logs the data at a 1-minute resolution, the data is actually integrated and used here at 1-hour resolution. This removes possible noise from the data. Still, obviously false data (such as gross outliers) was first removed at 1-minute resolution.

(14) p. 24403, l. 4: please add “. . . within the grid cells where the measurement sites are located. . .”. If this is what you were trying to say.

Added.

(15) P. 24403, l. 6: CDO mapping tool, either explain or give a reference for the reader who is not familiar with it.

Added at the end of second paragraph in section 3.

(16) p. 24405, l. 21f: “REMO's constant under-predictive. . .” More explanation is needed why “suddenly” you refer to the wet removal scheme.

Removed. It was indeed sudden.

(17) p. 24406, l. 17: include the information on the widths and number of intervals

Page 13 first paragraph includes information on how the bin widths were calculated. Equation 2.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

(18) Fig. 7: write the OVL in each box

Done

(19) p. 24407, l. 3f: move “and the monthly Z-score. . .” to the next paragraph where you describe the U-test. Include the number of samples that you apply the U-test for so the reader doesn’t have to calculate it

Moved. Page 14 first paragraph gives the information on number of samples.

(20) p. 24410, l. 5f: “This may hint to . . .” the logic is not clear. The authors say that BC is overestimated for this case and then conclude that residential wood-burning might be missing in the inventory. Please explain what you mean. Also the reference to the domestic heating source appears somewhat out of nowhere. There is no evidence in the data for this as you present it (see comment earlier, if there is potassium or levoglucosan to back up this assumption). This interpretation needs more thorough discussion.

Removed. Discussed in subsection 3.2.3.

(21) p. 24410, l. 8: “conservatively” what does this mean?

It refers to mass conservation.

(22) p. 24411, l. 10: “a clean Northern Finland atmosphere, “a statement about the whole tropospheric column cannot be made if only near-surface concentrations are considered. Be more specific here.

We introduced a sentence at the end of the sentence mentioned above, stating that: A similar pattern is found also for the cumulative concentrations over all vertical levels (not shown)

(23) p. 24412, l. 15: compare these values to the total emissions and/or concentrations over Finland to put it into context.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Done.

(24) Fig. 2: Indicate the year for the model run and the year of measurements for each plot.

Done

(25) Fig. 9 and 10: units for BC are missing

Added

(26) Technical Comments p. 24396, l. 20f: please rephrase: "...and biomass. Both natural processes and anthropogenic activities are responsible for the emission of BC. Black carbon particles absorb." p. 24397, l. 2: replace "assessing and . . ." by "resulting in an increase. . ." sulfate or sulphate? Biofuel or bio-fuel? p. 24400, l. 10: pluralize ". . . aerosol populations as well as their size-distributions and compositions." p. 24401 l. 28: ". . . (2011) and their locations are presented in Fig. 1." p. 24402, l. 13: "The time resolution of the instruments. . ." p. 24405, l. 15: delete "less-than-perfect" p. 24405, l. 23f: delete "dangerous" p. 24405, l 27: delete "attractive" p. 24409, l. 7: "diagrams" p. 24009, l. 9: ". . .represents the 3-h BC average mass concentration." p. 24410, l. 3: "omnipresent" doesn't work here. Please state more exactly what you mean p. 24410, l. 5: replace "fact" by "a result that. . ." p. 24411, l. 8: replace "the entire Finland" by "the whole country". p. 24412, l.19: "REMO-HAM" p. 24412, l. 26f: "... additional information on the agreement between shape and spread of the distributions of modeled and measured data." Fig. 4: before stars were used for the model, now it's squares. . . Fig. 5: caption: ". . .: model vs measurements." Fig. 12-14: include info "REMO-HAM for the year 2005"

All the technical errors were corrected, as suggested by the referee.

---

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 24395, 2012.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

